necessarily produce will not change. It will be that indicated by the arrows, namely, B D A. The direct and ordinary movement in the radiometer is thus explained in the simplest manner.

JOSEPH DELSAULX, S. J.

11, rue des Récollets, Louvain, Belgium, July 22

A Brilliant Meteor

LAST Tuesday evening, July 25, at three minutes past 10 P.M., a magnificent meteor was observed here. Its first appearance was hidden from me by a tree, but the rest of its long course was open to view. It travelled straight from S. to N. between the directions S.S.W. and W. Its apparent size was that of Jupiter. When first seen it was of a brilliant violet colour. This changed to bright green and red, and towards the end it was, I think, green in front, red behind, and where a number of globules which broke off seemed to follow it. The body of the meteor was pear-shaped. No luminous train was left after its disappearance. The motion was much slower than that of common aërolites, and probably the phenomenon lasted about two seconds. It would be interesting to know what was seen of it in the West of England and in Ireland.

Pembroke Lodge, Richmond Park, F. A. R. RUSSELL
July 28

ON Tuesday, the 25th, I was seated with my eyes looking westward, when at 10.5 P.M. a most remarkable meteor passed before my vision, which exceeded in brilliancy of colour and in dimensions any phenomenon of the kind that I ever witnessed in the whole course of my life.

The main body of the meteor was a vivid emerald green, with a large spherical head tapering away into a tail of fiery red colour, followed up by a luminous track.

Its trajectory was almost horizontal, emanating from the constellation of Aquila, passing through that of Hercules, curving slightly downwards, and passing a few degrees beneath Arcturus; a short distance northward of that great star the meteor suddenly collapsed with a bright effulgency, and vanished from sight.

Its velocity appeared as being somewhat slower that what I have observed on similar occasions. It was present to the observer for more than five seconds of time, sufficient time to leave on the mind of the observer a distinct impression of the meteor's various aspects.

Owing to the dry condition of the atmosphere, the apparent proximity of the meteor was very striking; the brilliant flash of colour at first sight produced the effect that a large rocket had been fired off in the vicinity, for it was very similar in colouring and shape to many rockets displayed by pyrotechnists.

Soon after the meteor had disappeared I observed three very faint shooting-stars to fall from a high altitude downwards to the track which the meteor had taken.

I furnish you with these observations, which may interest your readers, especially those who were fortunate enough to observe this splendid phenomenon. ERAS. OMMANNEY

6, Talbot Square, W., July 29

D-line Spectra

LAST March you were good enough to publish in NATURE (vol. xiii., p. 366) a request for some explanation of the extremely different, and indeed opposite, reactions afforded to *boric acid* by the yellow or D-line spectral flame emitted from soda or its salts, and from platinum respectively, when treated with the blowpipe.

No explanation has been vouchsafed; and it may be now added to that fact that, among the millions of substances in nature emitting this D-line producing-flame when heated before a blowpipe, sodium salts are the *only ones* which give the reactions of sodium; all others affording extremely marked reactions of an *exactly opposite* character. W. A. ROSS

July 24

Pyroxidation

WILL any of your chemist contributors be so kind as to afford in your columns an explanation of the following phenomenon?

If we heat before a blowpipe on a piece of aluminium plate (which has a side of four inches perpendicular to the blowpipe flame) a fragment of pure antimony, we have three sublimates deposited on the perpendicular side of the plate in the following order:—

(*a*) Sb_2O_5 (strongly reddening litmus paper) *highest.*
(*b*) Sb_2O_3 (faintly ,, ,, ,, intermediate.
(*c*) A *black* sublimate (?) lowest.

I want to know why a substance similar to another, except that it contains two more atoms of oxygen, and has therefore a higher specific gravity, travels perpendicularly up the plate to a more elevated position? W. A. ROSS

July 24

ABSTRACT REPORT TO "NATURE" ON EXPERIMENTATION ON ANIMALS FOR THE ADVANCE OF PRACTICAL MEDICINE[1]

V.

Results of Experiments on Resuscitation.

IN my last communication I described a method of practical study by experimentation which was intended to demonstrate the best means of restoring to life those human beings who by accident are thrown on the confines of death. To thoughtful and feeling minds this study is sublimely solemn, but I see that a writer in one of the contemporaries of NATURE has found it possible, in his zeal against experimentation on animals, to make my observations on the subject the matter of a jest at my expense. In order to render his jest applicable, the writer has also perverted my statement so as to make a simple illustration of a discovered fact appear as if it were presented in the light of the fact discovered. It will be remembered by the readers of these articles that after I had described, in my last essay, the observations relating to the effect of galvanism on expiring muscular power, I enforced the lesson by illustrating the difference of effect that might be expected to occur from carrying an exhausted animal to a place of succour and of making it travel to the place. The writer I refer to states this illustration as the fact which I have arrived at by experiment, and thereupon founds his joke, which he borrows from *Gil Blas.* The circumstance of this criticism has an interest for which I am thankful. It has suggested to my mind something which might not have occurred to it, viz., that in my desire to be very brief in these abstract reports I have neglected to introduce a few detailed arguments of first importance, which ought not, perhaps, to have been omitted in any case, but which I am now compelled to supply.

After the discovery of the process known as galvanism, and the researches conducted by Galvani, Volta, and Aldini on the influence of the galvanic current on animal life, the application of the current for the purpose of resuscitating persons who were apparently dead became the common practice of medical men. The extraordinary experiments conducted by Aldini at the College of Surgeons during the day of January 17, 1803, on the body of a malefactor named Forster, made an impression on men of science which was probably without parallel. The malefactor, after being hanged and after being exposed for a whole hour to a temperature two degrees below freezing-point, was carried to a house near to Newgate, and, in pursuance of the sentence, was delivered over to the College of Surgeons. The master of the College, Mr. Keate, here re-delivered the body over to Aldini, who was the nephew and devoted follower of Galvani, and the action of the galvanic current upon the dead man was demonstrated. I need not describe minutely the strange phenomena that were observed during the demonstration. Carpue, the anatomist, took share with his pupil Hutchins in the anatomical part; Cuthbertson, an eminent mathematical instrument-maker, the Browning of that day, directed and arranged the galvanic apparatus, which consisted of three troughs of forty elements each; Mr. Keate took duty in observing, and Aldini directed the operations. Fifteen experiments were carried out, and such were the muscular movements excited in the dead man by the current that the most sanguine expectations were afterwards expressed as to the power of galvanism to restore suspended life. Aldini, indeed, made a kind of apologetic observation that his "object in applying the treatment to the dead malefactor was not to produce reanimation, but merely to obtain a practical knowledge how far galvanism might be employed as an auxiliary to other means in attempts to revive persons under similar circumstances." The observations of Aldini were as impressive as they were remarkable. They opened a new line of inquiry and of research; but in their very wonder lay a source of many errors, and from these errors sprang a false rule of practice, which was unfortunate in its results for a very long time.

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[1] Continued from p. 252.

One error consisted in attributing to the action of galvanism something more than its power of calling forth the natural remaining irritability of the muscles of those in whom the signs of life are suspended. It was noticed that the muscles of the malefactor retained their irritability and power of contraction under the galvanic stimulus for seven hours and a half after the execution. The credit of this, which was due entirely, as my experiments have since explained, to the cold to which the dead body was exposed, was given to the galvanism; here was a second error. The greatest error was conveyed in an inference drawn, and I think naturally drawn, by Aldini, at that time, that the "application of galvanism gives new energy" to the muscles, and therefore that galvanism ought to have first place in the practice of resuscitation. "The well-known method," he says, "of injecting atmospheric air (artificial respiration) ought not to be neglected; but here, likewise, in order that the lungs may be prepared for its reception, it would be proper previously to use galvanism, to excite the muscular action, and to assist the whole animal system to resume its vital functions."

It was all but impossible that teaching such as this, backed as it was by experiment so remarkable, should fail to exert an influence on the practice of medicine in the treatment of suspended animation. It did, in fact, exert the most potent influence. It threw artificial respiration, which had been projected by Hooke, from experiment on animals, and which had been strongly urged by Fothergill and John Hunter, into the shade, and it gave to galvanism for nearly half a century the first place as the means, not simply for calling into motion the remaining energy of the dying muscles, but, as Aldini imagined, for "giving new energy" to the muscles. From the date of that theory, the battery, and after it was discovered, the electro-magnetic machine became the instruments of instruments for resuscitation.

Thirty years ago, when I was commencing my medical career, the application of the galvanic battery in cases of sudden death from drowning, from suffocation, and from other similar forms of sudden accident, was still the approved practice. The mode of operation was to place one pole of the battery at the nape of the neck, and the other pole below the diaphragm, and by passing shocks through the inclosed parts, to excite the muscles to contraction, so as to restore the movements of respiration. The effect was for a little time very startling; it seemed as if the natural function were called again into play; but in the end the motion excited became feebler and feebler; at last the stimulus failed, and the patient was declared to be dead. I recall many instances of this kind. I know of one instance of suspended human life from accidental suffocation, in which, when the natural breathing was just becoming restored by artificial inflation of the chest, the arrival of a battery and the application of it to expedite recovery was followed by complete cessation of all motion in response to the stimulus, and by absolute death. I know of another instance in which a needle from one pole of the battery was carried down to the heart, under the hope of exciting motion from the centre of the circulation, but, as it was said, "without avail."

In this position the method of resuscitation by means of galvanism stood until my experiments on animals recently dead from anæsthesia commenced. In experimenting with the galvanic current I was desirous of making it more precise in action, my original idea being that the constant failure of it as a means of recovery was due, not to fault in principle, but to some mistake of detail. With the research in this direction came the observation, altogether unexpected, that galvanism, even when it is made to reproduce the natural movements of respiration with such precision that they tally completely with the natural respiratory acts of the animal as those were counted and measured while the animal was in health, not only fails to restore the natural respiration in the majority of cases, but in the majority of cases destroys the respiratory power altogether. In brief, the experiments showed that the theory of Aldini, that galvanism "gives new energy" to the muscles was wrong, while the fact came out that the effect of the galvanism is only to whip into silence the muscles that are already well-nigh dead. This, which was found true in respect to the muscles concerned in respiration, was found to be equally true in respect to the heart.

The correction in matter of principle deduced, the comparison followed between artificial respiration carried out with a perfect instrument and the effect of galvanism, in the same forms and modes of death. Therewith followed the result that in extreme states where recovery is nevertheless all but certain by the process of artificial respiration supplied from the hand of the operator, death is all but certain from the application of the galvanic stimulus. The lesson taught by experiment was thus doubly valuable; it exposed the failure of a deceptive and fatal agency for means of restoration; it prompted the improvement of rational and successful means of restoration.

As the experiment with galvanism on the failing muscles of the lower animal opened my eyes to read the real facts, the reason came vividly enough before me, why in the human subject I had seen, with pain beyond measure or expression, the vigorously stirred muscular mechanisms sink under galvanic stimulation into irrevocable rest. Then I could point out and correct the error. In the absence of the experiment, the correction had been impossible. No man on a mere speculation would have dared to withhold from a dying patient the application of galvanic stimulation, until the danger of the practice was proved by experimental science. Yet how solemn is the issue let one example tell! Before the experiments I have related were performed and the new order of facts were elicited by them, I should—in the case of that child, whose history was told in my last communication, and who recovered by means of artificial respiration when the natural respiration had ceased and all the signs of death were developed—have tried, from the practice I then knew, to excite the respiratory power by galvanism, and should have believed, whatever had been the result, that the practice was, under the circumstances, the best that could have been employed. Now I know that the galvanic current would have killed the child outright, as surely as I know that the artificial respiration raised him back into life.

Aldini reports that after the observations on the malefactor, Forster, were concluded, Mr. Keate, the Master of the College of Surgeons, proposed to make comparative experiments on animals. If this had been done at that time and the relative merits of artificial respiration supplied by the power of the operator, and of artificial respiration excited by galvanism from the muscles of the affected subject had been compared, the original error of Aldini that the galvanic current "gives new energy" would at once have been detected, and it would have been seen that the current does no more than disperse the flickering power which the dying muscles retain. As far as I can ascertain no such comparison was instituted, and so, for nearly half a century, a practice prevailed which must have been constantly taking away the last chances of human life, while a truly saving practice,—artificial respiration,—remained without an improvement from the time of John Hunter, in last century, to that of Marshall Hall, who, in our own days, gave it new and prominent importance.

A dozen painless and carefully-conducted experiments made on inferior animals which were exposed at any moment to be knocked on the head for food, to be killed or mortally maimed with shot, or to be hunted to death in the field or warren, would have taught, in 1803, that the passage of a galvanic current through the muscles of a body recently dead confers on those muscles no new energy; that the current in its passage only excites temporary contraction; that the force of contraction resident in the muscles themselves is but educed by the excitation, and that to strike the life out of the muscles by the galvanic shock without feeding the force, expended by contraction, from the centre of the body is a fatal principle of practice. The experiments unfortunately were not performed, and the error, therefore, fatal as it was, continued without question, until my own unexpected observations revealed it in the light of an error and made it so self-evident that the illustration through which it may best be explained, admitted of being treated, by one who was wise after the event, as a subject for jest.

> "Vidi ego, naufragium qui riserat, æquore mergi."

I will not copy the comment of the poet: far more congenial to me were it to save the endangered life.

It is from experiences such as I have given above, and in many instances, that the necessity for experimentation on the lower animals forces itself on the minds of the members of the medical profession, and especially on the minds of those who are most earnest to remove fatal errors of practice and to devise saving methods. If it were only kept steadily in view that we medical men are always dealing with fatal accidents and fatal diseases; if it were only kept steadily in view that we are always asking ourselves—Is this we are doing for the best? Or, as new light dawns on us:—May this we are doing be for the worst rather than for the best, and may the old practices taught to us have rested on a false foundation? If these things were thought of, then our position would be better understood and our actions more correctly appreciated. I believe those who are most severe upon us would be most considerate under this discipline of reason if they would give it trial, and that the very impulses of kindness, I will even say of tenderness, that lead many to oppose experimental inquiry would actually make them experimentalists if they could once realise the highest responsibilities that devolve on the medical scholar. Nay, I am not without hope that my jesting critic himself, if he ever had to stand, as we physicians have to stand, over the body of one of his fellow men, who, in the midst of health had just passed into doubtful death: if this critic, I say, had to stand there wondering what he should do to recal the life, uncertain whether what he was about to do were for the best or the worst; he, I think, would lay aside *Gil Blas*, would be humanely tempted, to risk the sacrifice of the life of a lower for that of the higher animal, and would transfer the rabbit he had provided for his dinner, to the experiment room instead of the kitchen.

BENJAMIN W. RICHARDSON

(*To be continued.*)

OUR ASTRONOMICAL COLUMN

HUTH'S "MOVING STAR" OF 1801–2.—At the beginning of the present century, when, although Bode and some few others had been looking forward to such a discovery, astronomers generally were startled by Piazzi's accidental detection of the small planet Ceres, we read of observations of more than one so-called "moving star," which, after progressing slowly for a short interval, finally disappeared. The most singular narrative refers to an object said to have been remarked by Hofrath Huth, at Frankfort-on-the-Oder, on the night from December 2 to 3, 1801, particulars of which were communicated to Bode in several letters during the ensuing five weeks. If the observations are *bonâ fide*, there is yet a mystery attaching to the object to which they relate. Huth was one of the three independent discoverers of the periodical comet now known as Encke's, on October 20, 1805, Pons and Bouvard sharing with him an almost simultaneous discovery, and he did other astronomical work. Writing to Bode on December 5, he says: "In the night from the 2nd to the 3rd of this month, I saw with my $2\frac{1}{2}$-feet Dollond, in a triangle with θ and δ Leonis to the south-west, *a star with faint reddish light, round, and admitting of being magnified.* I could not discern any trace of it with the naked eye; it had three small stars in its neighbourhood." He writes again on the 15th, that unfavourable weather had allowed of his observing the object only on three occasions, which appear to be on the early mornings of the 3rd, 13th and 14th, and he concludes from his observations that it had a slow retrograde motion to the south-west. From the 13th to the 14th, by eye-estimate, it had retrograded 4′ of arc, and from the 3rd to the 13th at most 30′. He forwarded to Bode at this time a diagram of the neighbouring telescopic stars. On December 21 he writes again that he had only succeeded in observing his moving star on one additional night, that of December 19–20, when he found it "near four stars apparently situate to the westward, about half a diameter of the full moon below a smaller one." Its path appeared directed towards i Leonis and towards the ecliptic. He adds: "Of the motion of this planet-like star I can now no longer doubt, since I have observed a difference of $\frac{5}{6}^{\circ}$ nearly, between its positions on the 3rd and 20th." In a fourth letter, dated 1802, January 12, he informs Bode that he had seen the star on two later nights, those of the 1st and 2nd of the same month from 11h. to 14h., with many telescopic stars in its vicinity, of which he enclosed a diagram, by eye-estimate only, with the path of the object.

He mentions that on January 1 the star was even smaller than one of the satellites of Jupiter, and on the following night he had difficulty in perceiving it in close proximity to a star towards which it was moving. On the 5th he could discern only now and then, to the right of the star, on the left of which it was situated on the 1st and 2nd of January, and at a very small distance from it, a glimmer, but the star's former place on the left was vacant. He concludes that the object must have been receding from the earth, and might perhaps have been more distinct and larger before December 3. On the night of January 6 there was no trace of it. He closes this final letter by saying that he would have gladly learned that some other astronomer had observed this star and confirmed its motion, and expressing his regret that Bode had not succeeded in finding it. On the latter point Bode remarks that the weather during December had been but very rarely favourable for observation, and in the few moments that the sky was clear he had occupied himself with his "Seeker" and Dollond, partly in giving attention to the neighbourhood of Huth's star, and partly to the region in which Ceres was expected to be recovered on her second appearance. He also remarks on the imperfect manner in which the star's positions had been communicated to him, but concludes that "without doubt it was a distant comet," and its great distance caused it to appear without nebulosity. He supposes it on December 3 to have been in longitude 156° 20′, with latitude 10° 40′ north, and on January 2 in 154° 20′, with latitude 8° 50′. Huth's rough diagrams are reproduced in the *Berliner Jahrbuch*, 1805,